SCIENCE.

FRIDAY, AUGUST 20, 1886.

COMMENT AND CRITICISM.

THIS AND THE TWO SUCCEEDING numbers of *Science* will be largely given up to the reports of the meeting of the American association at Buffalo. In this number is given the address in full of the retiring president, Prof. H. A. Newton of Yale, and, with this, abstracts of several of the vice-presidents' addresses. We are also able to present our readers with a portrait of Prof. Edward S. Morse, of Salem, the incoming president. Professor Morse, was born at Portland, June 18, 1838. His career as a scientific man is one of the results of the enthusiasm aroused by the elder Agassiz, Professor Morse being one of the well-famed group of young Americans who came about Agassiz during his first years in this country. Professor Morse's investigations of the molluscoids, worms, and lower arthropoids, his marked success as a lecturer in biology, his enthusiastic study of Japan and the Japanese, which he has partially set forth in his admirable 'Japanese homes and their surroundings,' are the works which lead us to congratulate the association on their choice.

CAPITALISTS AND LABORERS.

THE adjustment of the relations between capitalists and laborers is the greatest problem presented for solution in the present age. It is one that has baffled the skill of the wisest men in times past. There is a bitterness and alienation between these classes that threaten the peace of society and the stability of government. There are millions of discontented people to a greater or less extent under the influence of socialists, who openly publish doctrines subversive of all good government, and contrary to religion and morality. Their leaders are bold and reckless, and avow their purpose to disturb society in order to make what they call a just division of property.

Quotations, from writers worthy of confidence, were given, in order to show that the condition of the laborer is far better in all respects than it was fifty years ago. In the increase of wages, and the lessening of the hours of toil, he gains from fifty to one hundred per cent in money returns. Advantages of education, comforts, and privileges, and means of relief from sickness and pain, that formerly were unknown, are now common. This improvement in the condition of the poor gives no reason for the haughty rebukes of their employer, nor for his advise to them to be content with their condition. With all our boasted advantages of modern civilization, the condition of a large portion of the laboring classes is pitiable. Thousands have no employment, and thousands more are compelled to live on a mere pittance, and submit to conditions destructive of all manhood and nobility of spirit.

In New York city there are two hundred thousand women and girls employed in ninety-two trades. They earn from four to eight dollars per week. Hundreds of cases are reported where women work from fourteen to seventeen hours per day at from four to seven dollars per week. Loss of time, from ill health and inability to obtain work, reduces their earnings till they barely sustain life. Many persons receive twelve and a half cents a day. Many of them are wronged, and on various pretexts deprived of their pay. The rules of many factories are abusive and degrading. The home life of such laborers is pitiable, being passed in circumstances where decency and womanly respect are impossible. About nineteen thousand tenement houses accommodate about fifty persons each, and some of them three times as many. The condition of a large number of the poor is a reproach to our age.

It is a sore evil that has resulted from the effectiveness of machinery. It separates the wage-workers into a permanent class, making it more and more difficult for them to rise above it.

The improved condition of the laborer makes him more restless, gives him new views and higher wants, which he seeks to gratify. He now longs for more rational living, better food, better clothing, a better house, the education of his children, and time for self-improvement. While his circumstances have improved, he sees greater improvement in those of others. The product of the union of capital and labor is greater than formerly, and the laborer demands as his just due a larger share.

The causes of discontent among laborers are serious and wide-spread. One cause is the difference in views as to the relations between em-

Abstract of an address before the section of economic science of the American association for the advancement of science at Buffalo, Aug. 19, by the Rev. Joseph Cummings, LL.D., of Evanston, Ill., vice president of the section.

Faithfully yours
Edw S Morse

ployers and employed. Labor has been degraded and despised. There is still a feeling that there must be fixed classes in society, and that the majority must work hard enough to relieve the minority from labor. Once it was the privilege of the employer to command, and the duty of the laborer to acquiesce; but this feeling of inferiority on the part of the employed is gone, and the age of civility is past. The workman has made practical the doctrine of human equality, and looks on those around him as his equals. He no longer respects any distinctions founded on birth and circumstances and not on personal worth and power. He holds truly that labor is service for an equivalent, and that the employer and employed stand as equals in an interchange of service. He does not admit that wages are paid by the employer, but regards them as the product of the joint effort of the employer and employed, of which the laborer should receive his just proportion. In fact, the employer has no more right to dictate to the laborer how he shall seek his interests, and what associations he shall form, and what trades-unions he shall establish, than the laborer has to dictate to the employer in corresponding matters. A great part of the alienation between classes, and the bitterness of the poor toward capitalists, lies in the fact that wages have been substituted for all other ties, and the laborers are regarded but as a part of 'the plant' in a great manufacturing establishment. In American society there is a marked manifestation of the degradation of labor. All labor which involves personal attention, and especially labor in household service, is still thought degrading. The term 'servant' is still used, but it should be banished from a civilized people, and become as obsolete as 'slave' and 'serf.'

There are serious errors that in some form have been advocated by leading political economists, which, under the teachings of such modern popular writers as Henry George, have caused serious evil. They are such maxims as this: that "all wealth is created by labor, and the title to all wealth ought to be vested in the laborers who have produced it." These maxims are fallacious; but they are received with great favor by the multitude, who are led to believe that the accumulation of great fortunes is a wrong to the laborers, and that such fortunes should be divided for the public good.

For the discontent of the laborers, and their disagreement with the capitalists, various remedies have been proposed, but they have proved, in practice, vain and ineffective. This may be said of strikes, lock-outs, and the doctrine of unrestricted competition. A reasonable mode for the settlement of difficulties would seem to be a conference between the classes or their representatives. When a settlement cannot thus be reached, it would seem the wisest course to refer the points in dispute to arbitrators chosen in the usual way. Boards of arbitration may be either temporary or permanent. There are many reasons in favor of permanent boards, which might be as effective in preventing difficulties as in their settlement.

We are persuaded that the present difficulties that threaten the peace and order of society will never be removed till a higher standard of ethics shall prevail. They are the direct result of selfishness, encouraged by the prevalent selfish theory of morals. These are personal sins and social wrongs that civil government may not by law or force correct. It is not according to the will of God, as made known by natural or revealed religion, that a few should control vast fortunes, using them to gratify selfish personal desires, while multitudes suffer not only for want of knowledge, but of bread, and struggle through a brief existence, realizing in no proper sense the true object of life. Nothing is right that is not in accordance with the divine will; hence no man can have the right, though he has the power, to do wrong. Because a gifted man has power to accumulate property, he has no right to arrogantly say, "This is mine and I will spend it as I please." The wealth of the world is designed for the public welfare; and it is the duty of those who have it in charge to consider themselves as only agents, bound to use it so as to serve the greatest good. He who has wealth and does not intend to act thus, is false to his trust, and is the enemy of society.

In the Christian use of money will be found the great remedy for social wrongs. The right use of money will require much tact, wisdom, and skill. Multitudes on multitudes of the poor have low, selfish, sensual aims; and indiscriminate giving to them would only encourage indolence and vice. They need education and culture, and higher ideas of life. All these the right use of money now worse than wasted would secure.

AN INVENTORY OF OUR GLACIAL DRIFT.

AFTER an introduction, and a reference to recent acquisitions in the field of geography and other departments of geology, the southern limits of the great glacial formations of North America were sketched and illustrated by wall map. In addition to the already known limits in the east, new facts were given respecting the outline in Dakota and Montana, the line being found to pass

Abstract of an address before the section of geology and geography of the American association for the advancement of science at Buffalo, Aug. 19, 1886, by T. C. Chamberlin, vice president of the section.

nearly due west from the latitude of Bismarck to within forty miles of the Rocky Mountains, where it curves rapidly to the north, and skirts the mountains as far into British America as yet traced. Within the United States the limit of north-eastern drift barely touches that of demonstrative local glaciation from the Rocky Mountains. Westward of this, in the valleys of Flathead, Pend D'Oreille, and Osoyoos lakes, and Puget Sound, are deposits of drift regarded as prolongations of the more general drift of British Columbia, which, if not a continuous mantle, at least passes beyond the character of simple local mountain drift. South of this general drift are deposits of ancient glaciers in the Cascades, Sierras, Rockies, some of the intermediate ranges, and, according to some authorities, the Appalachians. The lacustrine deposits of the great basin region were correlated with the glacial deposits in time and causation.

A wealth of significance lies in the sinuosities, vertical undulations, and varying characters of the southern border. It undulates over the face of the land essentially as much as an arbitrary line from New York harbor to Puget Sound, and could be reduced to horizontality — as it must have been to have marked the margin of some ancient ice-bearing body of water — only by incredible warpings and dislocations. The border of the drift presents three notable phases; one part terminating in a thickened belt, a terminal moraine; another in a thin margin; and a third in an attenuated border of scattered pebbles. The morainic border prevails in the Atlantic region, and lies on or near the limit as far west as central Ohio, beyond which it retires from it. Throughout the rest of the stretch to the Rocky Mountains the attenuated edges prevail. The latter are thought to represent, one a glacial and the other a glacio-natant action. The attenuated borders are believed to delimit an earlier ice incursion, and the morainic border a later one, which overrode the former in the coast region but fell behind it in the interior, having its extension in similar moraines in the interior.

Corroborative testimony is found in facts drawn from orographic attitudes, drainage, erosion, decomposition, ferrugination, vegetal accumulations, and lacustrine oscillations in the great basin. The interval between the two epochs is measured geologically by the cutting down of the beds of the Allegheny, Monongahela, and upper Ohio rivers some two hundred to three hundred feet, chiefly in rock; of the upper Missouri River to greater depth; and by an elevation of the upper Mississippi of eight hundred to one thousand feet. Of the earlier drifts, two important subdivisions seem indicated by present data, and several subordinate ones of the later. The distribution of these was outlined. A third series of drift sheets, of greater uniformity of material and regularity of deposition, occupying the great basins of the St. Lawrence valley, the Red River of the North, and limited areas of the coast region, and delimited in part by beach ridges, was sketched. The major opinion concerning the oldest series favors their glacier origin, but this opinion is not unanimous. Concerning the second, or moraine-bordered group, opinion is overwhelming that they are direct glacier products. Concerning the third series, the weight of opinion favors their subaqueous deposition, either in fringing lakes or in more general submergence. The differentiations of the characters of the three groups were further sketched. Of unstratified bowldery clays or tills, there is the richest variety, ranging through varying combinations of material, texture, and aggregation. Three genetic classes were recognized: 1°, subglacial tills; 2°, englacial or superglacial tills; 3°, subaqueous tills; and 4°, tills ridged by the thrust of the margin of the ice.

Of moraines, terminal, lateral, medial, and interlobate varieties are found. The great terminal moraines overshadow all others in interest and importance. The distribution of the chief ones were shown upon the map. The Nantucket and Cape Cod moraines were regarded, with more confidence than ever, as the equivalents of the Kettle Range of Wisconsin, and the Altamont and Gary moraines of Dakota. Outside of these chief moraines, there are occasional belts of older drift aggregated in the similitude of peripheral moraines. Examples are found in central Indiana, western Montana, and the plains of the British Possessions. Back from the two principal terminal moraines lie several similar partially determined belts, usually of less prominence and continuity.

Our most unique moraines are the interlobate, developed between the tongues into which the great ice sheet of the second epoch was divided at its margin. About a dozen of these, located in half as many states, were recognized; but only a part present full evidence of true interlobate character. Beautiful lateral moraines abound in the mountainous regions of the west, and some were developed by local glaciation supervening upon the ice retreat of the east. Our medial moraines are unimportant, and confined essentially to mountainous glaciation. Allied to the true moraines are special forms of aggregation of the subglacial debris, among which were enumerated: 1°, till tumuli; 2°, mammillary and lenticular hills; 3°, elongated parallel ridges, trending with the ice movement; 4°, drift billows; 5°, crag and

tail; 6°, pre-crag and combings; and 7°, veneered hills. The most remarkable are the mammillary, lenticular, and elongated ridges, frequently grouped under the term 'drumlins.' The lenticular varieties prevail in southern New Hampshire, central and eastern Massachusetts, north-eastern Connecticut, and Nova Scotia; the elongated variety, accompanied by shorter, in central New York; and all varieties in eastern Wisconsin, extending into the northern peninsula of Michigan. About three thousand have been mapped. The total known number probably aggregates ten thousand. No theory of their formation has yet received wide acceptance, beyond a general agreement that they are subglacial accumulations.

Turning to the assorted drift, two classes commonly embraced there were excluded. First, the 'orange sands' of the Mississippi valley, commonly accepted as Champlain deposits. They do not appear to possess the distinctive characteristics of glacial gravels, but are residuary in aspect. If they belong to the glacial period at all, it must be to its earliest stage. Their reference to the Champlain epoch is clearly an error. The second class, set aside as not being strictly glacial, were those reworked by wholly non-glacial agencies; or, in other words, the secondary drifts. Eliminating these, there remain the products of glacial waters working co-ordinately with the ice, of which two classes were recognized: 1°, those that gathered immediately within and beneath the ice body itself, or against its margin; and 2°, those which were borne to distances beyond its limit by the glacial drainage or by peripheral waters. In the first, the presence and restraint of the ice was an essential factor; in the second, it was only a source of material. Of the first class, there are: 1°, the products of streams flowing on the surface of the ice; 2°, of streams plunging from the surface to the base through crevasses; 3°, of subglacial streams in tunnels beneath the ice; 4°, of streams in ice cañons at the border; and 5°, debouchure deposits of streams at the margin. The products embrace a great variety of sub-types of gravel heapings, including isolated mounds, conical peaks, clustered hummocks with inclosed pits and basins, and sharp, steep-sided ridges, often of phenomenal length—all possessing great irregularities of material and stratification, embracing, frequently, manifest disturbances. The elongated variety,—identical in all essential respects with the great osars of Sweden,—are finely developed in eastern New England, especially in Maine, and the border of New Brunswick; while the hummocky variety, constituting the ill-defined class of kames, are abundant throughout New England, New York, northern New Jersey, Pennsylvania, Ohio, Indiana, the greater part of Michigan, northern Illinois, eastern and northern Wisconsin, northern Minnesota, north-central Iowa, eastern Dakota, and many portions of Canada. These osars and kames are among the most fascinating phenomena of the drift; but to differentiate them, and to determine to what extent they are superglacial, subglacial, and debouchure phenomena, is a triumph of discrimination not yet attained. It is of most practical importance at present to distinguish debouchure and submarginal gravel heapings, representative of the position of the glacier's edge, from the gravel veins of the glacier's body. The semi-morainic kames are the type of the one; the winding windrows of gravel, the osars, of the other. The osars frequently end in osar fans, and the kames graduate into pitted gravel plains. These pitted plains and others, not identical in type, constitute one of the singular and not least puzzling features of the assorted drift. They have a wide range; but find their most phenominal development in Wisconsin, Michigan, Ontario, and the coast of New England. The kames also graduate into true moraines; and every stage of gradation may be observed. In the progress of their accumulation, they were thrust by the adjacent ice, and heaped into ridges as genuinely morainic as though made of unwashed material. They have an especial development along the interlobate tracts.

Of valley drift formed by streams heading on the glaciers, the intermediate phases were passed with simple reference, and attention directed to two extreme phases: 1°, the moraine-headed valley trains; and 2°, the loess tracts. The former are deposits of glacial floods, when the slope gave impetus to the drainage; the latter were construed as the products of slack drainage. The former are found to show progressively coarser material toward their origin, and to merge into elevated expanded heads blending with the moraines from which they took their origin. Associated with these are glacial aprons of overwash drift, that fringe the outer sides of moraines in favorable situations. These phenomena point unequivocally to a glacial origin, and to vigorous drainage conditions. Contrasted with them are the broad tracts of fine silt, designated 'loess,' that occupy the Mississippi up to east-central Minnesota, the Missouri up to southern Dakota, the Illinois and Wabash as far up as their great bends, and the Ohio up to south-eastern Indiana. They are so correlated with the border of the ice, in the later stages of the earlier epoch, that they seem clearly to be products of glacial drainage of a fluvio-lacustrine character, indicating low gradients and slack drainage. This stands in marked contrast to the

conditions necessarily indicated by the moraine-heading coarse gravel streams ; and herein lies an important discrimination of the drainage and orographic attitudes of the two glacial epochs.

In addition to the till-like phases previously noted, two assorted deposits were considered. They range in altitude from below the sea-level to three thousand feet and beyond, and vary greatly in individual extent. The great examples are the immense sheets of assorted drift overspreading the great basins of the St. Lawrence, and the Winnipeg basin. These often present, among their surest credentials, overflow channels to the southward, crossing divides often hundreds of feet above existing outlets, and varying in altitude among themselves at least two thousand feet. Some of the more important were enumerated. Reference was also made to the iceward termination of these lacustrine deposits, a phenomenon yet but partially studied. The surfaces of these ancient lakes not only stood at altitudes greatly different from the present, but were tilted, if not distorted, as compared with existing water levels, rising as a general rule, toward the north. Data are being rapidly gathered, in the effort to determine how much of this was due to ice attraction, to ice weighting, to thermal changes, to intercurrent crustal changes independent of glacial presence, and to other and undiscovered causes. Reference was made to the scorings which the glacial floor presents, and some of the more remarkable features alluded to. The number of recorded observations of striae reaches nearly three thousand.

Turning to the more purely intellectual products springing from the glacial phenomena, it was noted that our former ample assortment of theories of the origin of the drift has become practically reduced to one,—the glacial. With few exceptions, the investigators of glacial phenomena in the United States accept as demonstrated the glacial origin of the greater mass of the drift. This is less true of Canadian investigators. Subordinate to this dominant hypothesis, there are various degrees of belief respecting the extent of auxiliary glacio-natant agencies.

Our wealth of working hypotheses has increased as our theory of genesis has become fixed upon the fruitful doctrine of the glacier origin of the drift. The recent introduction of strictly glacial methods has been prolific in stimulus and in interpretation. The working hpotheses necessary for the tracing out of moraines, the discrimination of the tills, the differentiation of the kames, osars, and similar products, and for the analysis of the drainage phenomena, have become rich beyond the limits of convenient statement, and suggestive to a degree unimagined a decade since. Under these, the advance of a year is becoming as the advance of a decade.

If we turn to the broader speculations respecting the origin of the glacial epoch, we find our wealth little increased. We have on hand practically the same old stock of hypotheses, all badly damaged by the deluge of recent facts. The earlier theory of northern elevation has been rendered practically valueless ; and the various astronomical hypotheses seem to be the worse for the increased knowledge of the distribution of the ancient ice sheet. Even the ingenious theory of Croll becomes increasingly unsatisfactory as the phenomena are developed into fuller appreciation. The more we consider the asymmetry of the ice distribution in latitude and longitude, and its disparity in elevation, the more difficult it becomes to explain the phenomena upon any astronomical basis. If we were at liberty to disregard the considerations forced upon us by physicists and astronomers, and permit ourselves simply to follow freely the apparent leadings of the phenomena, it appears at this hour as though we should be led upon an old and forbidden trail,—the hypothesis of a wandering pole. It is admitted that there is a *vera causa* in elevations and depressions of the earth's crust, but it is held inadequate. It is admitted that the apparent changes of latitude shown by the determinations of European and American observatories are remarkable, but their trustworthiness is challenged. Were there no barriers against free hypotheses in this direction, glacial phenomena could apparently find adequate explanation; but debarred—as we doubtless should consider ourselves to be at present—from this resource, our hypotheses remain inharmonious with the facts, and the riddle remains unsolved.

THE ECONOMICAL ASPECT OF AGRICULTURAL CHEMISTRY.

PROFESSOR WILEY opened his address with statistics showing the value of the agricultural products of the United States. He then gave figures showing the chemical constitution of the different products, and laid stress upon the necessity of supplying the growing crops with sufficient potassium, phosphorus, and nitrogen. The value of the potash, phosphoric acid, and albuminoids or nitrogen entering into a single harvest he estimated as follows, valuing potash at five cents per pound, phosphoric acid at six cents, and nitrogen at eighteen cents. The total value of each of these ingredients is, then, potash, $598,067,446;

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phosphoric acid, $418,865,930 ; nitrogen, $2,326,-852,674 ; total. $3,343,786,050.

These quantities of plant food removed from the soil annually seem enormous, but it must be remembered that they are not all lost: much of them is left in the soil in roots, straw, stalks, etc. Those, however, who are acquainted with the method of farming practised in the newer parts of our country know that corn-stalks and straw are generally regarded as nuisances, to be removed as easily and speedily as possible. It is not tilling but killing the soil that is practised. Stables are removed to get out of the way of the accumulating manure, and the corn-stalks are raked together and burned to prepare the field for a new crop. True, in many localities the waste of such a proceeding, especially in nitrogen, is understood. Yet it must be confessed that over vast areas of our agricultural lands there is no conception of the idea of possible exhaustion of the soil, and no systematic method of preventing it. The refuse of the crop, the straw, the stalks, etc., are put out of the way as easily and quickly as possible, and without thinking of the robbery which is thereby committed. The stores of plant food which have accumulated in our virgin soils are indeed great, but they cannot withstand this constant drain on them. The effects of this system of culture soon show themselves in diminished yield, as is seen in the great wheat fields of the north-west and of California, which do not produce at the present time more than half the crop at first obtained from them.

If we place at 40 pounds the annual contribution of potash of an acre of land to the crop, the number of crops which could be produced in a given depth, as far as this constituent of soil is concerned, is easily computed. The weight of dry soil per acre to a depth of nine inches is approximately 3,000,000 pounds. A soil containing .3 per cent of potash would have, therefore, 9,000 pounds, which, at 40 pounds a year, would last for 250 years. But fortunately, by the decomposition of feldspathic rocks and others containing potash, and also by the transfer in various ways of the subsoil to the soil, a provision is found which will prevent the entire exhaustion of the soil. Thus it happens, that, in many parts of the world where fields have been under cultivation for hundreds of years, there is still a sufficient amount of this manurial substance to insure the production of a crop.

Further, it must not be forgotten that there are many manurial substances containing potash which are accessible, and which will furnish immense stores of this substance to the future agriculturist. Chief among these natural deposits must be mentioned the mines of kainit, which have their greatest development near Stassfurt. These mines have already furnished immense quantities of potash, and there is no immediate danger of their exhaustion.

The available quantity of phosphorus as plant-food may be estimated in the same way. The quantity of phosphoric acid in soils varies from none at all to almost one per cent. If we take the mean content of phosphoric acid in a soil to be .15 per cent, the total quantity per acre to a depth of nine inches would be 4,500 pounds. If the contribution to each crop is 20 pounds per acre, the phosphoric acid would last for 225 years without any artificial supply.

The stores of phosphoric acid, however, which a provident past has saved for us, are even greater than the deposits of potash. Apatite is a somewhat abundant mineral; and in South Carolina and Alabama, and other states of the union, are found large beds of phosphates. Some idea may be formed of the extent of these deposits by studying the dimensions of the largest bed of them yet discovered, having its centre at Charleston, S. C. This bed has been traced for a distance of 70 miles parallel with the coast, and has a maximum width of 30 miles. In view of the fact that only preliminary surveys have been made of the phosphatic beds in North Carolina, Alabama, and Florida, and that these surveys have shown the presence of immense quantities of these deposits, it is just to conclude that the mineral wealth of the country, in this particular, is of no mean proportions.

The quantity of phosphates imported into the United States (not including guano) has diminished with the increase of home production, having fallen from 133,955 tons, worth $1,437,442, in 1883, to 27,506 tons, worth $367,333, in 1885.

For the fiscal year ending June 30, 1885, there was exported from the United States farm products having a value of $530,172,835. The value of agricultural products imported was $249,211,-975, more than half of which was sugar, tea, and coffee. The excess of exports over imports was therefore $280,960,860.

It must be remembered, however, that the values of exports are given at the seaboard, and are fully 25 per cent greater than for the values given at the farm. To compare, therefore, exports with total production, the sum above given must be diminished by one-fourth, becoming $397,629,626, or 11 per cent of the total net value of the farm production of the country. Allowing for the small quantities of valuable plant-food introduced in our agricultural imports, we may safely place the loss of these ingredients, due to exportation, at 10 per cent of the whole.

The exportation of agricultural products, becomes, therefore, a slow but certain method of securing soil exhaustion; and this accounts for the fact that countries — or those portions of countries which are devoted to almost exclusive agricultural pursuits, thus causing a continuous exportation of agricultural products — become the homes, not of the richest, but of the poorest communities.

It would be useless to deny in this connection that our own country, with a soil enriched by centuries of accumulated nitrogen, has grown rich from its agricultural exports. But when the last of our virgin soil shall have been placed under cultivation, a continuous stream of such exports will certainly impoverish the nation, and reduce all who practise such agriculture to the condition which has already been reached by those who have for years grown tobacco, corn, cotton, and wheat on the same soil, and sold the products without paying back to the field the percentage of profits which was its due. On the other hand, the farmer who is fortunate enough to be permitted to patronize the home market, who sells his maize and takes home a load of manure, adds not only to the plethora of his purse, but also to the fertility of his soil.

Thus, in the light of agricultural chemistry, we see clearly the deep scientific basis of the teachings of political economy which show the value of the home market. While, therefore, the statement that the chief factor in the prosperity of a country is its agriculture, remains in every sense true, yet, from the data discussed, it as readily appears that agricultural prosperity is most intimately connected with the advancement of every other industry. Agricultural chemistry teaches the farmer to welcome the furnace and the mill, for in their proximity he secures a sure return to his fields of the plant-foods removed in his crops.

We have seen by the foregoing discussion, that, without any artificial additions, the soil, excluding the subsoil, contains enough of the two most important and valuable mineral constituents of plants to produce an average crop annually for two hundred and fifty years. In point of fact, however, the impoverishment of the soil takes place at a much slower rate than this theory would indicate. It would indeed be a sorry thought to consider that in a quarter of a millennium more the agricultural area of the earth would be incapable of producing further yields. Doubtless much of this reserve food is brought from the subsoil; and, if it be possible for the subterraneous stores of these materials to gradually work their way surfacewards, even the remote future need not fear a dearth of them.

There is also a certain conservatism in crops, a vegetable 'good breeding,' which prevents the growing plant from taking all the food in sight. As long as there is abundance, the plant is a hearty eater; but, when the visible quantity of food falls to a certain minimum, it remains for a long time without any rapid diminution. This fact is well illustrated in the experiments of Lawes and Gilbert at Rothamstead, where wheat was grown on the same unmanured field for forty years in succession.

Professor Wiley then passed to a discussion of the sources of supply of nitrogen used as plant-food, and, after giving an extended account of the most recent researches, summed up the results as follows: —

1. The combined nitrogen, which is the product of vegetable and organic life, forms the chief source of nitrogen for the growing plant.

2. Before it is assimilable by the plant it undergoes a process of oxidation, which is due solely to a living organism.

3. The nitrates thus formed are absorbed by the plant, and the albuminoids of the new growth are formed from the nitric nitrogen by a process of reduction. The nitrates themselves are subject to the action of a ferment, by which a deoxidation takes place, and free nitrogen and nitrous oxide are evolved.

4. The diminution in the quantity of available nitrogen thus supplied is restored by the fixation of free nitrogen by the action of organisms in the soil, or by the oxidation of free nitrogen by the interior cells of the plant acting in a manner analogous to the nitric ferment in the soil; or by the oxidation of free nitrogen by electrical discharges or by combustion.

5. The quantity of combined nitrogen brought to the soil and growing plant by the rain-water and the atmosphere, arising from the last two phenomena, is an inconsiderable amount, when compared with the whole weight required by the crop.

Since, with a proper economy, the natural supplies of potash and phosphoric acid may be made to do duty over and over again, and last indefinitely, the economist, who looks to the welfare of the future, need have no fear of the failure of these resources of the growing plant. Indeed, it may be said that the available quantities of them may be increased by a wise practice of agriculture based on the teachings of agricultural chemistry.

But with the increase of population comes an increased demand for food, and, therefore, the stores of available nitrogen must be enlarged to supply the demands of the increased agricultural product. It is certain that, with new analytical

methods, and the new questions raised by investigation, many series of experiments will be undertaken, the outcome of which will definitely settle the question of the entrance of free nitrogen into vegetable tissues. If this question be answered affirmatively, agricultural science will not place bounds to the possible production of foods. If the nitrofying process does go on within the cells of plants, and if living organisms do fix free nitrogen in the soil in a form in which at least a portion of it may be nitrified, we may look to see the quantities of combined nitrogen increase *pari passu* with the needs of plant life. Thus, even intensive culture may leave the gardens and spread over the fields, and the quantities of food suitable for the sustenance of the human race be enormously increased.

In regarding the agricultural economies of the future, however, it must not be forgotten that a certain degree of warmth is as necessary to plant development as potash, phosphoric acid, and nitrogen. If it be true, therefore, that the earth is gradually cooling, there may come a time when a cosmic athermacy may cause the famine which scientific agriculture will have prevented. Fortunately, however, for the human race, the cereals, the best single article of food, are peculiarly suitable to a cold climate. Barley is cultivated in Iceland, and oatmeal feeds the best brain and muscle of the world in the high latitudes of Europe.

It is probably true that all life, vegetable and animal, had its origin in the boreal circumpolar regions. Life has already been pushed half way to the equator, and slowly but surely the armies of ice advance their lines. The march of the human race equatorwards is a forced march, even if it be no more than a millimetre in a millenium. Some time in the remote future the last man will reach the equator. There, with the mocking disc of the sun in the zenith, denying him warmth, flat-headed, and pinched as to every feature, he will gulp his last mite of albuminoids in his oatmeal, and close his struggle with an indurate in hospitality.

NOTES AND NEWS.

ACCORDING to the report of Gustavus Hinrichs of the weather service of Iowa, that state, since the middle of May, has been subjected to a drouth, the most severe on record. The most serious drouth preceding the present one prevailed during June and July of 1863, when for sixty days no serviceable rains fell in Iowa City; but rains had been sufficiently abundant till the end of May, and nearly five inches of water fell during the first ten days of August. In the early summer of 1886, the last good rain fell on May 13. After that time, there was no rain reaching half an inch until August 4, — eighty-three days without a serviceable shower! The total rainfall during that period was less than one inch, while the normal rainfall would be nearly ten and a half inches. But, notwithstanding this extreme drouth, it cannot be said that there is a failure of crops; because farming operations in that state are so diversified that a total failure is almost an impossibility.

LETTERS TO THE EDITOR.

****Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Glaciers and glacialists.

THE number of *Science* for the 23d of July last contains a paper by Mr. Jules Marcou, in which he refers to my memoir on Professor Guyot (published by the U. S. national academy), and denies statements cited by me from a publication by Professor Guyot with regard to the latter's glacier discoveries. Mr. Marcou commences his criticism on the subject with the following paragraph: "At Princeton Guyot was long isolated from intercourse with Swiss naturalists; and at the close of his life, while suffering under the malady which proved fatal in 1884, he put forth claims of doubtful value. These are the facts." Then follow the facts as Mr. Marcou understands them.

Mr. Marcou's statement is wrong in important points. Professor Guyot gives an account of his own discoveries of 1838 in his memoir of Professor Agassiz, which was read before the national academy, the first part in October, 1877, the second in April, 1878. This is six years before his decease, while he was still engaged in his laborious topographical survey of the Catskills. The following is the paragraph from the Agassiz memoir: —

"In the spring of 1838 I had the pleasure of a visit from my dear friend Agassiz in Paris, where I then resided. The main topic of conversation was, of course, the glaciers. He put me *au courant* of Charpentier's views, as yet imperfectly published (his book having been issued only two years later, in 1840), and adding his own idea of a general glacier era, he urged me to turn my attention to these phenomena. I asked to be allowed to suspend my judgment until my own observations should justify my adhesion to so startling a theory, but promised to visit the glaciers that very summer. I did so, and an exploring tour of six weeks in the Central Alps rewarded me beyond my expectation. The glacier of the Aar, on which Agassiz began two years later (1840) his regular system of observations, taught me the law of the moraines. The glacier of the Rhone gave me the law of the more rapid advance of the centre of the glacier, and that of the formation of the crevasses, both transversal and longitudinal. The glacier of Gries showed me the laminated, or ribboned (blue bands) structure of the ice deep down in the mass of the glacier, and the law of the more rapid advance of the top over the bottom. On the southern slope of Mont Blanc, the great glacier of La Brenva, with its twin rocks, rising like two dark eyes from the middle of the ice (they are, indeed, called by the mountaineers the 'eyes of the glacier'), made me understand that the motion of the glacier takes place by a gradual displacement of its molecules under the influence of gravity, giving it a sort of plasticity, and not

by a simultaneous gliding of its whole mass, as believed by de Saussure. All these laws, deduced from a first, but attentive, study of the phenomena of the glaciers, were, at that time, — excepting that of the moraines, — new for science. They were expounded by me, and illustrated by diagrams, at the meeting of the geological society of France, in session at Porrentruy, the same summer of 1838; and I had the great satisfaction of seeing them fully confirmed by the subsequent observations of Agassiz and others, which furnished the precise numerical data then wanting for their complete elucidation. This paper, however, though duly mentioned in the proceedings of the geological society (*Bulletin*, vol. ix. p. 407), was not printed, owing to a protracted illness of its author in the winter following. But on the occasion of a claim by Prof. J. D. Forbes to the discovery of the laminated, or ribboned, structure of the ice, the portion relative to this subject was printed, and the whole manuscript, on a motion of Agassiz, was deposited, by a formal vote, as a voucher, in the archives of the Society of natural sciences of Neuchatel, the original draft being now in my hands. If I mention this circumstance, it is because the regrettable omission of the publication of my paper was the occasion of the unfortunate misunderstanding which estranged two such men as Agassiz and Forbes, and which I feel bound, in a measure, to explain."

The manuscript referred to in the latter part of this citation was sent to the Society of natural sciences of Neuchatel early in 1883, was read at the session of the society on the 12th of April, 1883, and published in its *Bulletin* of the same year. I have a copy of the published paper, which I received from the secretary of the Neuchatel society. It is in French, as first written, and its title page, and also the cover, bears the heading, 'Observations sur les glaciers des Alpes en 1838, par M. Arnold Guyot.' I had thus, in Guyot's memoir of Agassiz, and this publication by the Society of natural sciences of Neuchatel, the fullest authority for my statements, and also, in this and other ways, abundant reason for confidence in Professor Guyot. Moreover, his memoir of Agassiz bears evidence throughout that his friendship for Agassiz, as I know from long and intimate intercourse with him, was, to the end, that of a brother.

In the same memoir, Guyot says of Venetz and Charpentier — names mentioned by Mr. Marcou — and of Agassiz's great results:

"If to Venetz and Charpentier belongs the honor of having first proved the transportation of the Swiss erratic bowlders by the agency of ice, and the existence of great glaciers formerly extending to the Jura, to Agassiz we must accord the merit of having given to these facts their full significance; of having brought them before the world at large, and having made the glacial question, as it were, the order of the day. By his sagacity he found glacial action where it was never suspected before, pointed it out to the astonished and unbelieving English geologists on their own soil; found it in North America; traced it with undoubted evidence in the temperate regions of South America; and believed, though with hardly sufficient reason, that he had seen it on the vast plains of the Amazon. He proved the phenomena to be well-nigh universal." Thus Guyot does justice to his friend, and recognizes the earlier work of Venetz and Charpentier.

My academic memoir of Guyot closes with the following sentence: "As fellow-students, we have special reason to admire in Guyot — as he wrote of Humboldt — 'that ardent, devoted, disinterested love of nature, which seemed, like a breath of life, to pervade all his acts; that deep feeling of reverence for truth, so manifest in him, which leaves no room for selfish motives in the pursuit of knowledge, and finds its highest reward in the possession of truth itself.'" I know this to be a just tribute.

Mr. Marcou's remark condemnatory of Professor Agassiz's 'successor at Harvard college,' for "having denied, *in toto*, in a publication founded by Agassiz, — 'The memoirs of the Museum of comparative zoölogy,'— his [Agassiz's] great discovery of the 'ice age,' but having, more than that, ignored him altogether as the discoverer of the existence of ancient glaciers in the British Dominions, in New England and New York, in Brazil, in the Straits of Magellan, and in Chili," is essentially groundless. 'The memoirs of the museum,' referred to, contain, among its volumes, a work entitled 'The climatic changes of later geological time, by J. D. Whitney,' and this is the only ground presented by Mr. Marcou for the charge he makes. Mr. Whitney's work opposes accepted views on ancient glacier distribution, and therein opposes Agassiz, and nearly all geologists living; but he has not a word of disparagement for Agassiz, and gives no just cause of personal complaint. Mr. Marcou's charge against Mr. Alexander Agassiz has no other foundation, and is not true to the views he holds, and has always held, with regard to his father's work and discoveries connected with glaciers and the 'ice-age.' The memoirs of the museum of comparative zoölogy, founded by Mr. Alexander Agassiz, and not by his father, has been for some time sustained, and the museum work carried on, with the grandly generous outlay on Mr. Agassiz's part of several hundred thousand dollars; and he has never made the stipulation, which the objector seems to require, that the publications should contain nothing in opposition to his own, or his father's opinions.

JAMES D. DANA.

New Haven, Aug. 11.

Lacustrine deposits of Montana.

The examination of the Gallatin valley in Montana, by the writer, under the supervision of Dr. F. V. Hayden, during the summer of 1885, has developed some points of general interest in relation to the old lake basins of that region. Dr. Hayden was the first to demonstrate the fact that the western country, during the tertiary period, was covered to a greater or less extent with lakes, the waters of which, as the tertiary period progressed, gradually changed from brackish to fresh; until in pliocene time there were numerous fresh-water lakes scattered all over the area of the west, from the Mississippi valley to the Pacific coast.

The first of the basins described by Dr. Hayden was the one lying east of the Rocky Mountains, and extending from the Niobrara River to an unknown distance south of the Platte River. He estimated that this lake must have occupied an area of from 100,000 to 150,000 square miles. To the beds deposited in this lake the name of the Loup Fork group was given; and they were found to shade imperceptibly into an upper group, to which he gave the name of Post-Pliocene, the lower strata having been

referred to the pliocene from a study of the abundant vertebrate remains found in them.

In 1871 the writer accompanied Dr. Hayden's expedition of that year from Utah across the Snake River plains, through Idaho to Montana. Old lake-beds were found filling many of the valleys,—the expansions of all the more important rivers and many of their tributaries having once been lakes. The lacustrine deposits consisted mainly of sands, arenaceous clays, and what were called 'marls.' They were recognized to be precisely like the beds of the Loup Fork group, generally light colored, white, creamy yellow, or ashy gray, and were referred by Dr. Hayden to the pliocene, from their lithological characters. He supposed that the lakes dated back to the pliocene, and that the waters gradually subsided during quaternary times. Specimens of the rocks were collected, in 1871 and 1872, and deposited in the Smithsonian institution. The only fossils obtained in 1871 were a fragment of the jaw of Anchitherium agreste, associated with a helix. Pliocene fossils were found by Dana and Grinnell, in 1874, in a basin on a branch of Smith's River in Montana, but the beds from which they were obtained were only fifty feet in thickness, and differ from the beds of the other lacustrine areas in the mountains. The data as to the age of these supposed pliocene deposits is therefore meagre. Some facts of rather recent discovery, however, seem to indicate that possibly their age, and their contemporaneity with the original Loup Fork beds and the post pliocene of Hayden, may eventually be established by the study of the lithological characteristics of the various basins. At any rate, certain lines of investigation are suggested that promise interesting results.

Somewhat more than a year ago Mr. George P. Merrill, of the national museum, informed me that in arranging the 'pliocene marls' and sands that I had collected in 1871, he was struck with their peculiar appearance; and upon subjecting them to a microscopic examination, he had found them to be composed mainly of volcanic material, several of them, in fact, being wholly composed of volcanic or pumiceous glass. These specimens were from some of the valleys in the upper branches of the Jefferson River. Although some of the coarser strata of the deposits were recognized in 1871 as being of volcanic origin, that the peculiar ashy gray, drab, and cream-colored beds so characteristic of the lacustrine areas were of a similar origin, was first demonstrated by Mr. Merrill's examinations. This discovery gave additional interest to the study of the Gallatin valley lacustrine area, which has been our field of study for the past two seasons.

The specimens collected in the summer of 1885 have also been examined, and they reveal the fact that the so-called marls and sands are composed largely of pumiceous glass, which was in all probability ejected into the air from volcanic vents, and deposited in the quiet waters of the lake. The character of the beds is such that they are very readily eroded and broken down, which probably accounts for the removal of so enormous a mass from the central portions of the valley. How great the thickness of the original deposit was, it is impossible to say exactly; still, the remnants on the south east side of the basin, near Bozeman, represent a thickness of at least eight hundred to a thousand feet. Only the eastern side of the valley has as yet been re-examined, and the beds are so generally concealed along the edge of the mountains that it is difficult to obtain a connected section in detail: the general section, however, has been determined. As in the case of the Loup Fork section, near the mountains, and in the lake-basins of other portions of Montana, there is a progression from calcareous beds up through loosely coherent sands to conglomerates, which cap the series. Creamy-white limestones with veins of quartz or chalcedony are the lowest rocks of the lake series in the Gallatin valley; and above them are light yellowish-gray, marly-looking sands, distinctly stratified. They are highly calcareous, but, after treatment with hydrochloric acid, the residue is found to be mainly made up of particles of glass. In the central portion of the section near Bozeman, the beds are composed almost purely of pumiceous glass, while the upper portions show a mingling of particles of crystalline rocks with the glass. The crystalline particles were evidently derived from the mountains near by, when they formed the shore of the lake. The pumiceous particles in the Gallatin valley specimens are sharp and angular, and show no evidence of attrition. The conclusion seems inevitable that this material was thrown into the air from some volcanic vent or vents, perhaps in repeated showers, and deposited in comparatively quiet waters. As the lake became more and more filled up, there appears to have been more agitation in its waters, and particles worn from the shores were mingled with the volcanic materials. That the latter was not carried in by water, seems probable, for the central portions of the beds are almost, if not entirely, made up of glass alone; and moreover, the finely comminuted condition of the particles, and their homogeneity in close proximity to the shore, confirm the view that they are wind-carried. The general resemblance of the Montana sections to those of the Loup Fork region led me to look up in the national museum some of the Loup Fork fossils collected by Dr. Hayden from 1856 to 1857, and described by Dr. Leidy. Sufficient material for microscopic examination was found adhering to many of the bones, and, in nearly every case, pumiceous particles were recognized in the sand. Specimens sent to the writer within the last three months, from several localities in northern and north-western Nebraska, and from north-western Kansas, have also contained similar volcanic glass. Mr. G. P. Merrill, in the 'Proceedings of the national museum for 1885' (p. 99–100), has described volcanic dust from southern Nebraska. Dr. M. E. Wadsworth (*Science*, vi. p. 63) describes similar material from south-east of the Black Hills in Dakota; and Prof. J. E. Todd discovered, in 1885, in eastern and north eastern Nebraska, beds of siliceous material, which were identified by Mr. J. S. Diller as being composed largely of volcanic glass (*Science*, vii. p. 373). We find, therefore, that not only is there a resemblance in appearance and in the order of succession between the Loup Fork beds and the lacustrine strata of Montana, but that in both, volcanic dust or pumiceous glass enters largely into their composition; and it is suggested that future investigations may possibly lead to a determination of their age through the careful study of this volcanic material.

The fresh-water tertiary formations east of the Rocky Mountains, and even in the mountains, have been supposed to differ from those in the west (in Idaho, Nevada, and Oregon), where the accumulation of volcanic sediments in the old lake-basins has been recognized by Newberry, King, Russell, Gilbert, and

others. Will we not, therefore, have to cut down very materially the great length of time generally believed to have elapsed in this region from the beginning of this lacustrine period to the present time, when we find that a great portion of the sediment that once filled the lakes is due, not to the products of erosion, as has hitherto been supposed, but to repeated showers of volcanic dust? Again, do not these volcanic materials, which must have fallen in showers over a large extent of country,—accumulating in some cases in beds forty to ninety feet thick, —account for the perfect preservation of the vertebrate remains which characterize the formations in so many parts of the west; and is there not also suggested one possible cause for the extinction of some of the many groups of animals which have at present no descendants in this region, and whose only remains are the bony fragments found in these lacustrine deposits?

A. C. PEALE.

U. S. geological survey.

Carnivorous prairie dogs.—Carnivorous orioles.

The statement of R. W. Shufeldt that his pair of young prairie dogs took kindly to a meat diet (*Science*, viii. p. 102) attracted my attention and interest, for it recalled to my mind an experience of my own in the summer of 1838. Having a pair of the marmots at this moment under observation here, I determined to try them with a piece of raw beef, and the eagerness with which they *plunged* at it (for their avidity cannot be characterized by any milder word) was certainly something very astonishing. Their ordinary vegetable food they take *quietly*, but the beef seemed to set them frantic. They acted as though they were famishing, — they seized it so fiercely, fighting with one another for it, and hastening back to ask for more. And so it has continued. Their owner fears to feed them with it exclusively, but gives them more or less daily, and the contrast between their eagerness for the meat and their quiet consumption of vegetables is a very instructive lesson. Their stomachs, out on the plains, always hold vegetable contents and nothing else. This was doubtless the first piece of meat ever tasted by either of these. Whence this craving appetite?

The experience of 1838 to which I referred was this: That was in the earlier days of my 'natural history,' three years before my first ichthyological paper was written. I had taken three young Baltimore orioles from their nest, but feared that I should lose them, for they refused every variety of food I offered them. At that time I was collecting birds zealously, and was skinning several of them daily. As I was preparing a specimen, one of the young orioles was sitting on my table, very stupid indeed, head drawn in, not life enough to utter a sound, thoroughly dumpish. Without knowing why, I picked up a bit of the bird's flesh and offered it to him. To my great surprise he swallowed it on the instant, and roused himself at once. That one mouthful had done him so much good that he wanted more. I took him on my finger and fed him piece after piece, till his throat was swelled out like an over-fed chicken's crop, and I feared to give him more. He settled himself down with great satisfaction, and went to sleep. I fed his brother and sister in the same way; and from that time till they were fully grown they had not a mouthful of food except the flesh of the birds I was skinning. Their eagerness for the meat was extreme. They learned the bird-skinning business to perfection. As soon as they saw me prepared for work, they all gathered about the specimen, ravenous for meat, and I almost always commenced to skin my bird, with an oriole sitting on each hand, and one on the specimen itself, and with three little heads down over the abdomen, where the first cut was to be made (they knew the point well enough); and the instant I opened the skin, in went three bills, digging and tearing fiercely for their food, and continuing at it as I continued my work, till their appetites were satisfied.

I do not know that this fact concerning the Baltimore oriole has ever been reported. I recollect mentioning it to Mr. Audubon, but it was after his account of the species had been published.

W. O. AYRES.

New London, Conn., Aug. 11.

Flooding the Sahara.

In our own country an evaporation of two feet per year is a small figure, and twice that amount has been recorded in some cases; so that it would seem to be safe to assume that it would exceed the latter value in the north of Africa. Taking Mr. LeConte's figures (*Science*, vol. viii. p. 35), and an evaporation of *two* feet per year, and the cubic feet evaporated, on an area of 3,100 square miles would be $2 \times 864.230 \times 10^5$ cubic feet $= 1,728,460 \times 10^5$ cubic feet per year. But the inflow, according to his assumptions, would be $1,262,277 \times 10^5$ cubic feet per year; so that at the rate of two feet of evaporation per year, the amount evaporated would be 1.3 times the amount of the inflow. In other words, at the rate of inflow assumed, the depression to be flooded would *never* be so far filled as to make a surface of 3,100 square miles: and if the evaporation be four feet per year, the inflow would necessarily be nearly three times that assumed by Mr. LeConte.

DE VOLSON WOOD.

Hoboken, Aug. 14.

Barometer exposure.

The discussions in *Science* relating to the effect of high winds upon the indications of a barometer in a room, have been highly interesting. I only desire at this time to present a few facts that bear upon the problem, and to correct a few misconceptions. No one that has attempted making a fire in a very cold room, on a very windy day, with a refractory chimney in the fore ground, can be easily convinced that there is much of a draft up a cold chimney, even with a hurricane. Even if there were such draft, the air must flow in through all the cracks, especially on the windward side, and equilibrium would thus be kept up. It should be noted also that the wind does not blow steadily, but rather in gusts; consequently there can be no such thing as a permanent lower pressure inside than outside a room, but a momentary depression by a gust would be relieved almost immediately by the lull.

This is shown beautifully by a barograph properly arranged. All references will be to a barograph inclosed in a tight glass case, such as has been adopted by Mr. Hough of Albany. The fluctuations are so rapid that they cannot be seen on a sheet carried at the rate of one to two inches per day, but only upon

one carried from seven-tenths of an inch to one inch per hour. In the latter case, with a very high wind sometimes, but rather the exception, there will be seen fine serrations, at intervals of one or two minutes, having the appearance of a very fine saw. These serrations are quite regular, and are seen only during the high wind. The greatest fluctuation cannot be more than eight one-thousandths of an inch and seldom are above four one thousandths to six one-thousandths. It is probable that the wind influences these fluctuations, but it is very difficult to determine just how. That a high wind does not always produce them is quite remarkable. Returning to our drawing chimney, it would seem an interesting computation as to how long a gust would need to last in order to draw out of a chimney one foot square sufficient air to produce the supposed depression.

If we consider that the barograph is inclosed in an almost air-tight case, we have still another addition to our problem. Even if there were a withdrawal of air from the room, is it possible for the influence to reach the inside of the case before the lull has made a change? A partial answer to this question may be had by experimenting with the case. If the door be opened rather suddenly a partial vacuum is formed, or a jar occurs, which moves the float, and the pencil falls or rises according as the barometer has previously had a tendency down or up. This effect is only two one-thousandths of an inch; and it is very rare that an influence greater than that can be brought to bear upon the apparatus under these conditions. It would seem as though the effect produced by opening or closing the case may be many times greater than the utmost that can come from an intermittent wind.

If we turn to the original letter by Mr. Clayton (vol. vii. p. 484), we shall find these particular cases given by him: 1°. "On March 16 the wind's velocity rapidly rose from five to thirty-five miles, and the barometer suddenly fell five one hundredths of an inch;" 2°, "During a sudden gust attending a shower, last summer, the barometer fell a tenth of an inch, and immediately rose again as the gust ended;" 3°, "It [the pressure] fell as much as a tenth of an inch during a seventy-mile wind in February." It will be seen that each of these cases occurred under abnormal conditions, and just at the time when we would naturally expect such fluctuations; but they can hardly be due to the wind, as they are often noted when there is no high wind. The wind's action is intermittent, and there is no evidence whatever of this most important fact making itself known. It is a matter of regret that Mr. Clayton did not open and shut his trap-door at intervals of five or ten minutes, for an hour or so. He would have settled the question beyond doubt if he had done this.

Much has been written in regard to the evidence of observations on Mount Washington. Mr. H. A. Hazen has given a partial discussion of the Mount Washington records in the 'Annual report of the chief signal officer,' for 1882. He there has shown that the effect of the wind upon the computed elevation changes sign at a velocity of twenty-five to thirty miles per hour; i. e., instead of the effect being zero when there was no wind, it was really zero with a wind of twenty-five to thirty miles per hour. This is a fair indirect proof either that the wind does not cause the fluctuation, or, if it does, that another force is superposed upon it.

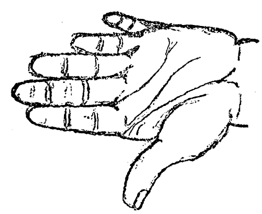
It is hazardous drawing conclusions upon the facts thus far developed. It may be that the wind can produce more than one effect, and that the serration effect above alluded to is not the only one to be considered. The weight of evidence seems to be rather against any great depression being produced. Mr. Clayton will do meteorology a great service by trying a few experiments. If his barograph, shut, is carried along only two inches a day, opening the trap-door ten minutes will give only one seventy-second of an inch for the pencil to move in. The difficulty can be obviated, however, by letting an attendant note the movement of the pencil (if there be any) and carefully take the time of the fluctuations, if the time of manipulating the trap-door be also taken, a comparison of times will settle the question.

Gan.

Aug. 10.

A case of inherited polydactylism.

In the spring of 1883 I saw and examined a case of inherited polydactylism, which I think worth recording. While enjoying the hospitality of a friend, in a charming ravine opening into Napa Valley from the mountains on the west side, my attention was drawn, by my intelligent hostess, to the hands of a German laborer at work in the garden. There were six well-formed, usable fingers on each hand. The metacarpals were of the normal number, but the fifth bore two fingers. The supernumerary little finger differed from the true little finger only in being much smaller.



I give a rude drawing of the left hand, made on the spot, showing the size and position of the supernumerary finger.

I inquired concerning his family history in this regard. His account is given in the following diagram, in which I have italicized those who are or were polydactylous:

Maternal Grandfather. *Maternal Grandmother.*

Father. *Mother.*

Sisters. Bro.

Peter Weitner, *a*, b, c, d; *a.*

Children.

It is seen that the deformity was inherited from his mother's maternal grandmother; that, besides himself, it has affected one sister, out of four, and one brother, and has been transmitted to the children of the sister, thus affecting at least four generations.

Joseph Le Conte.

Berkeley, Cal., Aug. 5.

"Thumb marks."

One of the anatomical characteristics recently brought within the area of anthropological investigation is the marking on the skin of the hand, espe-

cially of the thumb. Indeed, a proposition has been made to use this characteristic for identifying the Chinese emigrants to California. In Germany, especially, attempts have been made to show that these markings have racial significance. Has it ever been noticed that this custom has been borrowed from China, where the thumb and finger markings are used for purposes of identification, and by illiterates in signing papers? In the 'Proceedings of the China branch of the Royal Asiatic society,' for 1847, p. 11, is an article on land-tenure in China, by Thos. T. Meadows. Appended to this article is a copy of a deed bearing the thumb-signature of the grantor, a woman. Chinese sailors shipping on junks are made to sign with five fingers, in order to get a more certain identification. Dr. D. B. McCartee informs us that the Chinese class the striae at the ends of the fingers into 'pots' when arranged in a coil, and 'hooks' when they form a curving loop. They say that two men's thumbs may be alike, but that it is hardly possible that their hands would make similar pot-hooks.

WALTER HOUGH.

U. S. national museum, Aug. 10.

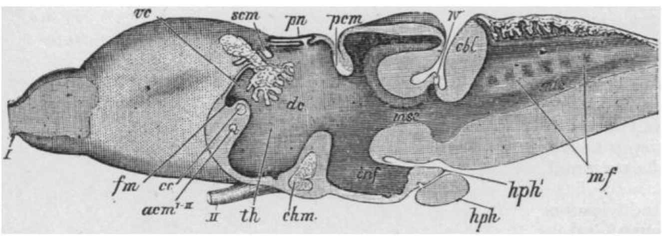


FIG 1.—A vertical section of the frog's brain (*Rana esculenta*): *acm*, anterior commissure; *cc*, or *cal*, corpus callosum; *dc*, or v^3, third ventricle; *fm*, foramen of Monro; *lt*, lamina terminalis; *pr*, pineal gland; *lv*, lateral ventricle; *vc*, ventriculus communis.

The corpus callosum in the lower vertebrates.

The corpus callosum, or great commissure of nerve fibres connecting the cerebral hemispheres, has long been one of the landmarks of comparative anatomy. In every modern work upon zoölogy, this commissure is given as a brain character which distinguishes the mammals from the lower orders of vertebrates. In fact, Owen long maintained that the corpus callosum proper was wanting in the marsupials and the monotremes; and his authority on this point was generally accepted until Flower, in 1865, demonstrated that this commissure is well developed in these animals, although much smaller in relation to the size of the anterior commissure. These observations were soon confirmed by Sander.

It is an interesting fact, as an example of knowledge apparently going backwards, that the earlier anatomists, in studying these commissures, hit much nearer the truth than their successors. For instance, that acute observer, Meckel, so long ago as 1816, correctly described the corpus callosum in the brain of the duck, and Reissner found it in the brain of the frog, in 1867. Other authors gave more or less accurate accounts of this organ in the lower vertebrates. More recently, in 1875, Stieda found it in the brain of the turtle. In face of these statements, all subsequent authorities, including Mihalkovics, Rl. Rückhard, Bellonci, and Stieda (with the exception of his one observation mentioned above), hold that the corpus callosum first arises among the mammals. This error, as it now appears, has sprung from two causes: first, from the difficulty of following the nerve-fibre courses in these small brains, a difficulty which has been to a great extent removed by improved microscopic methods; second, from the following fact: the anterior commissure in the mammalian brain consists of two divisions, one going to the olfactory lobes, the other to the temporal lobes. Recent authors have been led to confuse the commissure which really represents the corpus callosum, with the first-mentioned division of the anterior commissure, the truth being that the distribution of this commissure has never been precisely observed.

During the past winter I had an opportunity of studying the cerebral commissures in types of all the lower orders, in the most thorough manner; and found that the corpus callosum, so far from being a structure peculiar to the mammals, is present in the reptiles, birds, and Amphibia, and probably also in the Dipnoi and other fishes. In short, this commissure is a primitive character of the vertebrate brain. An account of the steps which led to this conclusion would exceed the due limits of this article, but an outline of the results may be given.[1]

For our present purpose, we must recall the embryonic position of the mammalian corpus callosum as a delicate bundle, traversing the thin wall which unites the hemispheres, and known as the 'lamina terminalis.' Below this, in the lamina, is another fibre-bundle, the anterior commissure. In the placental mammals, these bundles, from the time of their first development, are separated by an interval or

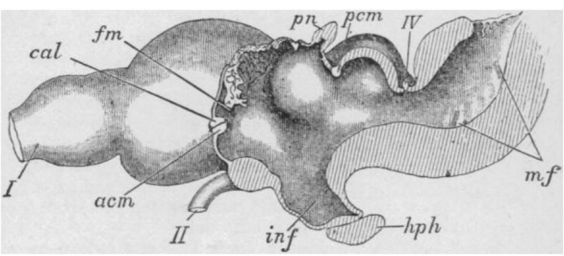


FIG. 2.—A vertical section of the turtle's brain (*Emys Europaea*).

septum; but in the marsupial brain, at an early stage, they lie close together in the middle line, very much as they are represented in fig. 3, in the turtle's brain (*cal* and *acm*), the upper bundle bending upwards, like a horseshoe; the lower passing outwards in the floor of the lateral ventricle (*lv*).

In the brain of the frog, in vertical section (fig. 1), we observe two bundles similarly placed in the lamina terminalis. The lowermost (*acm*) consists of two parts of unequal size, the larger part passing for-

[1] See *Morphologisches jahrbuch*, xii, August.

wards to the olfactory lobes, the smaller passing backwards. They correspond in distribution to the two divisions of the anterior commissure in the mammal. Does the upper bundle, then, represent the corpus callosum? When we follow the distribution

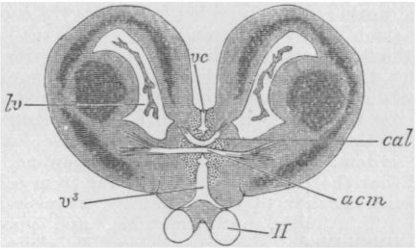


FIG. 3. — Transverse section of the fore-brain of the turtle in the plane of the cerebral commisures.

of its fibres to the upper inner cell-area of the hemispheres, this question seems clearly answered in the affirmative. But here arises a difficulty; this bundle lies below the foramen of Monro, and its fibres pass upwards *behind* the foramen, and then forwards above it. This is exactly the reverse of their position in the mammalian brain; but an explanation is found in the fact that the frog's brain retains many fish characters, and, among them, a large ventricle (the ventriculus communis) common to the two hemispheres, with the cerebral commissures lying in its floor. The brain of the turtle gives us a step nearer the mammalian type; for here, as in the mammal, the cerebral commissures lie in the front wall of the common ventricle, and the callosal bundle passes upwards in *front* of the foramen of Monro, and its fibres spread like rays over the entire inner wall of the hemispheres. Removing all further doubt that this bundle is homologous with the corpus callosum, is the fact that connected with it, as in the mammals' brain, are fibres passing backwards and downwards into a region which corresponds with the

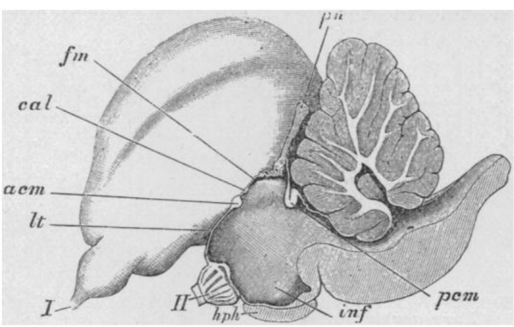


FIG. 4. — A vertical section of the brain of the duck (*Anas boschas*).

mammalian hippocampus. These fibres are usually described as the commissural portion of the fornix. The snake's brain (Tropidonotus) gives us a higher step, for, although the corpus callosum is a less distinct bundle, fibres are observed descending in the lamina terminalis, which in their relations closely resemble the columns of the fornix, — another structure which has been supposed to be peculiar to the mammals. In this brain also the olfactory and temporal divisions of the lower bundle have precisely the same relations as in the mammalian anterior commissure, demonstrating beyond a doubt that the lower bundle represents the entire anterior commissure, and not merely its temporal division, as Stieda and Mihalkovics contend. Upon drawing apart the hemispheres of the freshly removed brain of a duck, we observe a delicate thread of fibres slightly above a large and distinct lower commissure. The former, in transverse section (fig. 5), is seen passing directly upwards into the inner wall of the hemispheres, and below it is a powerful transverse commissure. We cannot fail to recognize that these two bundles are essentially similar in distribution and position to those in the turtle, and that the upper one is a rudiment of the corpus callosum.

Here is seen an apparent anomaly. In the frog's brain, the proportion of the corpus callosum to the anterior commissure is as 2 to 1; in the turtle it is about 5 to 4, while in the birds it is about 1 to 6. Thus, with an ascending scale of intelligence, we find a diminishing corpus callosum, a relation the reverse of that which obtains in the mammals. The

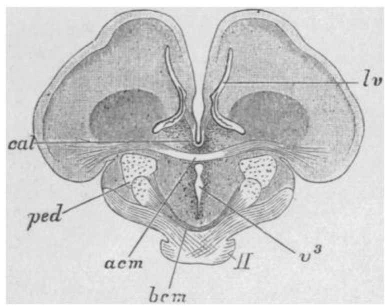


FIG. 5. — A transverse section of the duck's brain through the commissures.

explanation of this is probably that in the Sauropsida generally the inner wall of the hemispheres is thin, and in the birds it is reduced to a mere sheet of nerve-tissue, and this reduction of distribution area has effected a reduction of the commissure. In all these animals the united commissures are even smaller in proportion to the hemispheres than they are in the monotremes. HENRY F. OSBORN.

A brilliant meteor.

You may think worthy to record the following memoranda of an unusually large and brilliant meteor, reported by Mr. E. Stockin of Watertown, Mass., and seen from that place on Sunday evening, Aug. 8. Time, about 8.45 P. M. Direction, north-east to east. The attention of both Mr. and Mrs. Stockin was first called to the meteor by the flash, which illuminated surrounding objects. On turning, they saw the meteor, apparently about thirty degrees above the horizon. It was of a bright red color, of about one-fourth the size of the moon, occupying five or six seconds in its descent, disappearing behind some buildings while still brilliant, and leaving a trail of brilliantly colored sparks, and subsequently a white streak visible some seconds. The exact direction of the meteor from the observer could be ascertained, if desirable, by means of positions noted at the time. C. H. AMES.

Boston, Mass.

// SCIENCE.—SUPPLEMENT.

FRIDAY, AUGUST 20, 1886.

METEORITES, METEORS, AND SHOOTING-STARS.

You are kindly giving to me an hour to-night in which I may speak to you. I do not have enough confidence in myself to justify me in speaking to such an audience as this upon one of those broad subjects that belong equally to all sections of the association. The progress, the encouragements, and the difficulties in each field are best known to the workers in the field, and I should do you little good by trying to sum up and recount them. Let me rather err, then, if at all, by going to the opposite extreme.

Two years ago your distinguished president instructed and delighted us all by speaking of the pending problems of astronomy, what they are, and what hopes we have of solving them. To one subject in this one science, a subject so subordinate that he very properly gave it only brief notice, I ask your attention I propose to state some propositions which we may believe to be probably true about the meteorites, the meteors, and the shooting-stars.

In trying to interest you in this subject, so remote from the studies of most of you, I rely upon your sense of the unity of all science, and at the same time upon the strong hold which these weird bodies have ever had upon the imaginations of men. In ancient times temples were built over the meteorite images that fell down from Jupiter, and divine worship was paid them; and in these later days a meteorite stone that fell last year in India became the object of daily anointings and other ceremonial worship. In the fearful imagery of the Apocalypse, the terrors are deepened by there falling 'from heaven a great star burning as a torch,' and by the stars of heaven falling "unto the earth as a fig tree casteth her unripe figs when she is shaken of a great wind." The "great red dragon having seven heads and ten horns, and upon his heads seven diadems," is presented in the form of a huge fire-ball. "His tail draweth the third part of the stars of heaven, and did cast them to the earth." Records of these feared visitors, under the name of flying dragons, are found all through the pages of the monkish chroniclers of the middle ages. The Chinese appointed officers to record the passage of meteors and comets, for they were thought to have somewhat to say to the weal or woe of rulers and people.

By gaining in these later days a sure place in science, these bodies have lost their terrors; but so much of our knowledge about them is fragmentary, and there is still so much that is mysterious, that men have loved to speculate about their origin, their functions, and their relations to other bodies in the solar system. It has been easy, and quite common too, to make these bodies the cause of all kinds of things for which other causes could not be found.

They came from the moon; they came from the earth's volcanoes; they came from the sun; they came from Jupiter and the other planets; they came from some destroyed planet; they came from comets; they came from the nebulous mass from which the solar system has grown; they came from the fixed stars; they came from the depths of space.

They supply the sun with his radiant energy; they give the moon her accelerated motion; they break in pieces heavenly bodies; they threw up the mountains on the moon; they made large gifts to our geological strata; they cause the auroras; they give regular and irregular changes to our weather.

A comparative geology has been built up from the relations of the earth's rocks to the meteorites; a large list of new animal forms have been named from their concretions; and the possible origin of life in our planet has been credited to them.

They are satellites of the earth; they travel in streams, and in groups, and in isolated orbits about the sun; they travel in groups and singly through stellar spaces; it is they that reflect the zodiacal light; they constitute the tails of comets; the solar corona is due to them; the long coronal rays are meteor streams seen edgewise.

Nearly all of these ideas have been urged by men deservedly of the highest repute for good personal work in adding to human knowledge. In presence of this host of speculations it will not, I hope, be a useless waste of your time to inquire what we may reasonably believe to be probably true. And if I shall have no new hypotheses to give you, I offer as my excuse that nearly all possible ones have been already put forth. This as-

Address to the American association for the advancement of science at Buffalo, Aug. 18, 1886, by Prof. H. A. Newton, of New Haven, the retiring president of the association.

sociation exists, it is true, for the advancement of science, but science may be advanced by rejecting bad hypotheses as well as by framing good ones.

I begin with a few propositions about which there is now practical unanimity among men of science. Such propositions need only be stated. The numbers that are to be given express quantities that are open to revision and moderate changes.

1. The luminous meteor tracks are in the upper part of the earth's atmosphere. Few, if any, appear at a height greater than one hundred miles, and few are seen below a height of thirty miles from the earth's surface, except in rare cases where stones and irons fall to the ground. All these meteor tracks are caused by bodies which come into the air from without.

2. The velocities of the meteors in the air are comparable with that of the earth in its orbit about the sun. It is not easy to determine the exact values of those velocities, yet they may be roughly stated as from fifty to two hundred and fifty times the velocity of sound in the air, or of a cannon ball.

3. It is a necessary consequence of these velocities that the meteors move about the sun and not about the earth as the controlling body.

4. There are four comets related to four periodic star-showers that come on the dates April 20, August 10, November 14, and November 27. The meteoroids which have given us any one of these star-showers constitute a group, each individual of which moves in a path which is like that of the corresponding comet. The bodies are, however, now too far from one another to influence appreciably each other's motions.

5. The ordinary shooting-stars in their appearance and phenomena do not differ essentially from the individuals in star-showers.

6. The meteorites of different falls differ from one another in their chemical composition, in their mineral forms, and in their tenacity. Yet through all these differences they have peculiar common properties which distinguish them entirely from all terrestrial rocks.

7. The most delicate researches have failed to detect any trace of organic life in meteorites.

These propositions have practically universal acceptance among scientific men. We go on to consider others which have been received with hesitation, or in some cases have been denied.

With a great degree of confidence, we may believe that shooting-stars are solid bodies. As we see them they are discrete bodies, separated even in prolific star-showers by large distances one from another. We see them penetrate the air many miles, that is, many hundred times their own diameters at the very least. They are sometimes seen to break in two. They are sometimes seen to glance in the air. There is good reason to believe that they glance before they become visible.

Now these are not the phenomena which may be reasonably expected from a mass of gas. In the first place, a spherical mass of matter at the earth's distance from the sun, under no constraint, and having no expansive or cohesive power of its own, must exceed in density air at one-sixth of a millimetre pressure (a density often obtained in the ordinary air pump), or else the sun by his unequal attraction for its parts will scatter it. Can we conceive that a small mass of gas, with no external restraint to resist its elastic form, can maintain so great a density?

But suppose that such a mass does exist, and that its largest and smallest dimensions are not greatly unequal; and suppose further that it impinges upon the air with a planetary velocity; could we possibly have as the visible result a shooting-star? When a solid meteorite comes into the air with a like velocity, its surface is burned or melted away. Iron masses and many of the stones have had burned into them those wonderful pittings or cupules which are well imitated, as M. Daubrée has shown, by the erosion of the interior of steel cannon by the continuous use of powder under high pressure. They are imitated also by the action of dynamite upon masses of steel near which the dynamite explodes. Such tremendous resistance that mass of gas would have to meet! The first effect would be to flatten the mass, for it is elastic; the next to scatter it, for there is no cohesion. We ought to see a flash instead of a long burning streak of light. The mass that causes the shooting-star can hardly be conceived of except as a solid body.

Again, we may reasonably believe that the bodies that cause the shooting-stars, the large fireballs, and the stone-producing meteor all belong to one class. They differ in kind of material, in density, in size. But from the faintest shooting-star to the largest stone-meteor, we pass by such small gradations that no clear dividing lines can separate them into classes. See wherein they are alike: —

1. Each appears as a ball of fire traversing the apparent heavens, just as a single solid but glowing or burning mass would do.

2. Each is seen in the same part of the atmosphere, and moves through its upper portion. The stones come to the ground, it is true, but the luminous portion of their paths generally ends high up in the air.

3. Each has a velocity which implies an orbit about the sun.

4. The members of each class have apparent motions which imply common relations to the horizon, to the ecliptic, and to the line of the earth's motion.

5. A cloudy train is sometimes left along the track, both of the stone-meteor, and of the shooting-star.

6. They have like varieties of colors, though in small meteors they are naturally less intense and are not so variously combined as in large ones.

In short, if the bodies that produce the various kinds of fire-balls had just the differences in size and material which we find in meteorites, all the differences in the appearances would be explained; while, on the other hand, a part of the likenesses that characterize the flights point to something common in the astronomical relations of the bodies that produce them.

This likeness of the several grades of luminous meteors has not been admitted by all scientific men. Especially it was not accepted by your late president, Prof. J. Lawrence Smith, who by his studies added so much to our knowledge of the meteorites. The only objection, however, so far as I know, that has been urged against the relationship of the meteorites and the star-shower meteors, and the only objection which I have been able to conceive of that has apparent force, is the fact that no meteorites have been secured that are known to have come from the star-showers. This objection is plausible, and has been urged, both by mineralogists and astronomers, as a perfect reply to the argument for a common nature to all the meteors.

But what is its real strength? There have been in the last hundred years five or six star-showers of considerable intensity. The objection assumes that if the bodies then seen were like other meteors, we should have reason to expect that among so many hundreds of millions of individual flights a large number of stones would have come to the ground and have been picked up.

Let us see how many such stones we ought to expect. A reasonable estimate of the total number of meteors in all of these five or six star-showers combined makes it about equal to the number of ordinary meteors which come into the air in six or eight months. Inasmuch as we can only estimate the numbers seen in some of the showers, let us suppose that the total number for all the star-showers was equal to one year's supply of ordinary meteors. Now the average annual number of stone-meteors of known date from which we have secured specimens has, during this hundred years, been about two and a half.

Let us assume, then, that the luminous meteors are all of like origin and astronomical nature; and further assume that the proportion of large ones, and of those fitted to come entirely through the air without destruction, is the same among the star-shower meteors as among the other meteors. With these two assumptions, a hundred years of experience would then lead us to expect two, or perhaps three, stone-falls from which we secure specimens during all the half-dozen star-showers put together. To ask for more than two or three is to demand of star-shower meteors more than other meteors give us. The failure to get these two or three may have resulted from chance, or from some peculiarity in the nature of the rocks of Biela's and Tempel's comets. It is very slender ground upon which to rest a denial of the common nature of objects that are so similar in appearance and behavior as the large and small meteors.

It may be assumed, then, as reasonable that the shooting-stars and the stone-meteors, together with all the intermediate forms of fire-balls, are like phenomena. What we know about the one may with due caution be used to teach facts about the other. From the mineral and physical nature of the different meteorites, we may reason to the shooting-stars, and from facts established about the shooting-stars we may infer something about the origin and history of the meteorites. Thus it is reasonable to suppose that the shooting-stars are made up of such matter and such varieties of matter as are found in meteorites. On the other hand, since star-showers are surely related to comets, it is reasonable to look for some relation of the meteorites to the astronomical bodies and systems of which the comets form a part.

This common nature of the stone-meteor and the shooting-stars enables us to get some idea, indefinite but yet of great value, about the masses of the shooting-stars. Few meteoric stones weigh more than one hundred pounds. The most productive stone-falls have furnished only a few hundred pounds each, though the irons are larger. Allowing for fragments not found, and for portions scattered in the air, such meteors may be regarded as weighing a ton, or it may be several tons, on entering the air. The explosion of such a meteor is heard a hundred miles around, shaking the air and the houses over the whole region like an earthquake. The size and brilliancy of the flame of the ordinary shooting-star is so much less than that of the stone-meteor that it is reasonable to regard the ordinary meteoroid as weighing pounds, or even ounces, rather than tons.

Determinations of mass have been made by measuring the light and computing the energy needed to produce the light. These are to be regarded as lower limits of size, because a large part of the energy of the meteors is changed into heat

and motion of the air. The smaller meteors visible to the naked eye may be thought of without serious error as being of the size of gravel stones, allowing, however, not a little latitude to the meaning of the indefinite word 'gravel.'

These facts about the masses of shooting-stars have important consequences. The meteors, in the first place, are not the fuel of the sun. We can measure and compute within certain limits of error the radiant energy emitted by the sun. The meteoroids large enough to give shooting-stars visible to the naked eye are scattered very irregularly through the space which the earth traverses; but in the mean each is distant two or three hundred miles from its near neighbors. If these meteoroids supply the sun's radiant energy, a simple computation shows that the average shooting-star ought to have a mass enormously greater than is obtained from the most prolific stone-fall.

Moreover, if these meteoroids are the source of the solar heat, their direct effect upon the earth's heat by their impact upon our atmosphere ought also to be very great: whereas the November star-showers, in some of which a month's supply of meteoroids was received in a few hours, do not appear to have been followed by noticeable increase of heat in the air.

Again, the meteoroids do not cause the acceleration of the moon's mean motion. In various ways, the meteors do shorten the month as measured by the day. By falling on the earth and on the moon, they increase the masses of both, and so make the moon move faster. They check the moon's motion, and so, bringing it nearer to the earth, shorten the month. They load the earth with matter which has no momentum of rotation, and so lengthen the day. The amount of matter that must fall upon the earth in order to produce in all these ways the observed acceleration of the moon's motion, has been computed by Professor Oppolzer. But his result would require for each meteoroid an enormous mass, one far too great to be accepted as possible.

Again, the supposed power of such small bodies, —bodies so scattered as these are, even in the densest streams,—to break up the comets or other heavenly bodies; and also their power, by intercepting the sun's rays, to affect our weather, must, in absence of direct proof to the contrary, be regarded as insignificant. So, too, their effect in producing geologic changes by adding to the earth's strata has, without doubt, been very much over-estimated. During a million of years, at the present rate of, say, fifteen millions of meteors per day, there comes into the air about one shooting-star or meteor for each square foot of the earth's surface.

To assume a sufficient abundance of meteors in ages past to accomplish any of these purposes, is, to say the least, to reason from hypothetical and not from known causes. The same may be said of the suggestion that the mountains of the moon are due to the impact of meteorites. Enormously large meteoroids in ages past must be arbitrarily assumed, and, in addition, a very peculiar plastic condition of the lunar substance, in order that the impact of a meteoroid can make in the moon depressions ten, or fifty, or a hundred, miles in diameter, surrounded by abrupt mountain walls two, and three, and four miles high, and yet the mountain walls not sink down again.

The known visible meteors are not large enough nor numerous enough to do the various kinds of work which I have named. May we not assume that an enormous number of exceedingly small meteoroids are floating in space, are falling into the sun, are coming into our air, are swept up by the moon? May we not assume that some of these various results, which cannot be due to meteoroids large enough for us to see as they enter the air, may be due to this finer impalpable cosmic dust? Yes, we may make such an assumption. There exist, no doubt, multitudes of these minute particles travelling in space. But science asks not only for a true cause, but a sufficient cause. There must be enough of this matter to do the work assigned to it. At present we have no evidence that the total existing quantity of such fine material is very large. It is to be hoped that through the collection and examination of meteoric dust we may soon learn something about the amount which our earth receives. Until that shall be learned, we can reason only in general terms. So much matter coming into our atmosphere as these several hypotheses require would, without doubt, make its presence known to us in the appearance of our sunset skies, and in a far greater deposit of meteoric dust than has ever yet been proven.

A meteoroid origin has been assigned to the light of the solar corona. It is not unreasonable to suppose that the amount of the meteoroid matter should increase toward the sun, and that the illumination of such matter would be much greater near the solar surface. But it is difficult to explain upon such a hypothesis the radial structure, the rifts, and the shape of the curved lines, that are marked features of the corona. These seem to be inconsistent with any conceivable arrangement of meteoroids in the vicinity of the sun. If the meteoroids are arranged at random, there should be a uniform shading away of light as we go from the sun. If the meteoroids are in streams along cometary orbits, all lines bounding the light and shade in the coronal light should

evidently be projections of conic sections of which the sun's centre is the focus. There are curved lines in abundance in the coronal light, but, as figured by observers and in the photographs, they seem to be entirely unlike such projections of conic sections. Only by a violent treatment of the observations can the curves be made to represent such projections. They look as though they were due to forces at the sun's surface rather than at his centre. If those complicated lines have any meteoroid origin (which seems very unlikely), they suggest the phenomena of comets' tails rather than meteoroid streams or sporadic meteors. The hypothesis that the long rays of light which sometimes have been seen to extend several degrees from the sun at the time of the solar eclipse are meteor streams seen edgewise, seems possibly true, but not at all probable.

The observed life of the meteor is only a second, or at most a few seconds, except when a large one sends down stones to remain with us, What can we learn about its history and origin ?

Near the beginning of this century, when small meteors were looked on as some form of electricity, the meteorites were very generally regarded as having been thrown out from the lunar volcanoes. But as the conviction gained place that the meteorites moved not about the earth but about the sun, it was seen that the lunar volcanoes must have been very active to have sent out such an enormous number of stones as are needed in order that we should so frequently encounter them. When it was further considered that there is no proof that lunar volcanoes are now active, and that when they were active they were more likely to have been open seas of lava, not well fitted to shoot out such masses, the idea of the lunar origin of the meteorites gradually lost ground.

But the unity of meteorites with shooting-stars, if true, increases a hundred fold the difficulty, and would require that the comets have the same origin with the meteorites. No one claims that the comets came from the moon.

That the meteorites came from the earth's volcanoes is still held by some men of science, particularly by the distinguished astronomer-royal for Ireland. The difficulties of the hypothesis are, however, exceedingly great. In the first place, the meteorites are not like terrestrial rocks. Some minerals in them are like minerals in the rocks. Some irons are like the Greenland terrestrial irons. But no rock in the earth has yet been found that would be mistaken for a meteorite of any one of the two or three hundred known stone-falls. The meteorites resemble the deep terrestrial rocks in some particulars, it is true, but the two are also thoroughly unlike.

The terrestrial volcanoes must also have been wonderfully active to have sent out such a multitude of meteorites as will explain the number of stone-falls which we know, and which we have good reason to believe have occurred. The volcanoes must also have been wonderfully potent. The meteorites come to us with planetary velocities. In traversing the thin upper air, they are burned and broken by the resisting medium. Long before they have gone through the tenth part of the atmosphere the meteorites usually are arrested and fall to the ground. If these bodies were sent out from the earth's volcanoes, they left the upper air with the same velocity with which they now return to it. What energy must have been given to the meteorite before it left the volcano, to make it traverse the whole of our atmosphere and go away from the earth with a planetary velocity. Is it reasonable to believe that volcanoes were ever so potent, or that the meteorites would have survived such a journey ?

No one claims that the meteors of the star-showers, or their accompanying comets, came from the earth's volcanoes. To ascribe a terrestrial origin to meteorites is, then, to deny the relationship of the shooting-star and the stone-meteor. Every reason for their likeness is an argument against the terrestrial origin of the stones. To suppose that the meteors came from any planets that have atmospheres, involves difficulties not unlike to, and equally serious with, those involved in the theory of a terrestrial origin.

The solar origin of meteorites has been seriously urged, and deserves a serious answer. The first difficulty which this hypothesis meets, is that solid bodies should come from the hot sun. Besides this, they must have passed without destruction through an atmosphere of immense thickness. Then there is a geometric difficulty. The meteorite shot out from the sun would travel, under the law of gravitation, nearly in a straight line out and back again into the sun. If in its course it enters the earth's atmosphere, its relative motion, that which we see, should be in a line parallel to the ecliptic, except as slightly modified by the earth's attraction. A large number of these meteors, that is, most if not all well-observed fire-balls, have certainly not travelled in such paths. These did not come from the sun.

It has been a favorite hypothesis that the meteorites came from some planet broken in pieces by an internal catastrophe. There is much which mineralogists can say in favor of such a view. The studies of M. Stanislas Meunier, and others, into the structure of meteorites, have brought out many facts which make this hypothesis plausible. It requires, however, that the stone-meteor be not

regarded as of the same nature as the star-shower meteor, for no one now seriously claims that the comets are fragments of a broken planet. The hypothesis of the existence of such a planet is itself arbitrary; and it is not easy to understand how any mass that has become collected by the action of gravity, and of other known forces, should, by internal forces, be broken in pieces and these pieces sent asunder. The disruption of such a planet by internal forces, after it has by cooling lost largely its original energy, would be specially difficult to explain.

We cannot, then, look to the moon, nor to the earth, nor to the sun, nor to any of the large planets, nor to a broken planet, as the first home of the meteoroids, without seeing serious if not insuperable objections. But since some of them were in time past certainly connected with comets, and since we can draw no line separating shooting-stars from stone-meteors, it is most natural to assume that all of them are of a cometary origin. Are there any insuperable objections that have been urged against the hypothesis that all of the meteoroids are of like nature with the comets, that they are in fact fragments of comets, or it may be sometimes minute comets themselves? If such objections exist, they ought evidently to come mainly from the mineralogists, and from what they find in the internal structure of the meteorites. Astronomy has not as yet furnished any objections. It seems strange that comets break in pieces, but astronomers admit it, for it is an observed fact. It is strange that groups of these small bodies should run before and follow after comets along their paths, but astronomers admit it as fact in the case of at least four comets. Astronomically, there would seem to be no more difficulty in giving such origin to the sporadic meteor, and to the large fire-ball, and to the stone-meteor, than there is in giving it to the meteor of the star-shower. If, then, the cometic origin of meteorites is inadmissible, the objections must come mainly from the nature and structure of the meteoric stones and irons. Can the comet in its life and history furnish the varied conditions and forces necessary to the manufacture or growth of these peculiar structures?

It is not necessary, in order to answer this question, to solve the thousand puzzling problems that can be raised about the origin and the behavior of comets. Comets exist in our system, and have their own peculiar development, whatever be our theories about them. It will be enough for my present purpose to assume as probably true the usual hypothesis that they were first condensed from nebulous matter; that that matter may have been either the outer portions of the original solar nebula, or matter entirely independent of our system and scattered through space. In either case, the comet is generally supposed, and probably must be supposed, to have become aggregated far away from the sun. This aggregation was not into one large body, to be afterwards broken up by disruption or by solar action. The varieties of location of the cometic orbits seem inexplicable upon any such hypothesis. Separate centres of condensation are to be supposed, but they are not *a priori* unreasonable. This is the rule rather than the exception everywhere in nature.

Assume, then, such a separate original condensation of the comet in the cold of space, and that the comet had a very small mass compared with the mass of the planets. Add to this the comet's subsequent known history, as we are seeing it in the heavens. Have we therein known forces and changes and conditions of such intensity and variety as the internal structure of the meteorites calls for? What that structure is, and, to some extent, what conditions must have existed at the time and place of its first formation, and during its subsequent transformations, mineralogists rather than astronomers must tell us. For a long time it was accepted without hesitation that these bodies required great heat for their first consolidation. Their resemblance to the earth's volcanic rocks was insisted on by mineralogists. Prof. J. Lawrence Smith, in 1855, asserted without reserve that "they have all been subject to a more or less prolonged igneous action corresponding to that of terrestrial volcanoes." Director Haidinger, in 1861, said, "with our present knowledge of natural laws, these characteristically crystalline formations could not possibly have come into existence except under the action of high temperature combined with powerful pressure." The likeness of these stones to the deeper igneous rocks of the earth, as shown by the experiments of M. Daubrée, strengthened this conviction. Mr. Sorby, in 1877, said, "it appears to me that the conditions under which meteorites were formed must have been such that the temperature was high enough to fuse stony masses into glass; that the particles could exist independently one of the other in an incandescent atmosphere subject to violent mechanical disturbances; that the force of gravitation was great enough to collect these fine particles together into solid masses, and that these were in such a situation that they could be metamorphosed, further broken up into fragments, and again collected together."

Now, if meteorites could come into being only in a heated place, then the body in which they were formed ought, it would seem, to have been a

large one. But the comets, on the contrary, appear to have become aggregated in small masses. The idea that heat was essential to the production of these minerals was at first a natural one. All other known rock formations are the result of processes that involve water or fire or metamorphism. All agree that the meteorites could not have been formed in the presence of water or free oxygen. What conclusion was more reasonable than that heat was present in the form of volcanic or of metamorphic action?

The more recent investigations of the meteorites and kindred stones, especially the discussions of the Greenland native irons and the rocks in which they were imbedded, are leading mineralogists, if I am not mistaken, to modify their views. Great heat at the first consolidation of the meteoric matter is not considered so essential. In a late paper, M. Daubrée says: "It is extremely remarkable that, in spite of their great tendency to a perfectly distinct crystallization, the silicate combinations which make up the meteorites are there only in the condition of very small crystals, all jumbled together as if they had not passed through fusion. If we may look about us for something analogous, we should say that instead of calling to mind the long needles of ice which liquid water forms as it freezes, the fine-grained texture of meteorites resembles rather that of hoar-frost, and that of snow, which is due, as is known, to the immediate passage of the atmospheric vapor of water into the solid state." So Dr. Reusch, from the examination of the Scandinavian meteorites, concludes that "there is no need to assume volcanic and other processes taking place upon a large heavenly body formerly existing but which has since gone to pieces."

The meteorites resemble the lavas and slags on the earth. These are formed in the absence of water, and with a limited supply of oxygen, and heat is present in the process. But is heat necessary? Some crystallizations do take place in the cold; some are direct changes from gaseous to solid forms. We cannot in the laboratory reproduce all the conditions of crystallization in the cold of space. We cannot easily determine whether the mere absence of oxygen will not account fully for the slag-like character of the meteorite minerals. Wherever crystallization can take place at all, if there is present silicon and magnesium and iron and nickel, with a limited supply of oxygen, there silicates ought to be expected in abundance, and the iron and nickel in their metallic form. Except for the heat, the process should be analogous to that of the reduction of iron in the Bessemer cupola, where the limited supply of oxygen combines with the carbon and leaves the iron free. The smallness of the comets should not, then, be an objection to considering the meteoric stones and irons as pieces of comets. There is no necessity of assuming that they were parts of a large mass, in order to provide an intensely heated birth-place.

But although great heat was not needed at the first formation, there are many facts about these stones which imply that violent forces have in some way acted during the meteorites' history. The brecciated appearance of many specimens, the fact that the fragments in a breccia are themselves a finer breccia, the fractures, infiltrations, and apparent faultings seen in microscopic sections and by the naked eye—these all imply the action of force. M. Daubrée supposes that the union of oxygen and silicon furnishes sufficient heat for making these minerals. If this is possible, those transformations may have taken place in their first home. Dr. Reusch argues that the repeated heating and cooling of the comet, as it comes down to the sun and goes back again into the cold, is enough to account for all the peculiarities of structure of the meteorites. These two modes of action do not, however, exclude each other. Suppose, then, a mass containing silicon, magnesium, iron, nickel, a limited supply of oxygen, and small quantities of other elements, all in their primordial or nebulous state (whatever that may be), segregated somewhere in the cold of space. As the materials consolidate or crystallize, the oxygen is appropriated by the silicon and magnesium, and the iron and nickel are deposited in metallic form. Possibly the heat developed may, before it is radiated into space, modify and transform the substance. The final result is a rocky mass (or possibly several adjacent masses), which sooner or later is no doubt cooled down throughout to the temperature of space. This mass, in its travels, comes near to the sun. Powerful action is there exerted upon it. It is heated. How intense is that heat upon a cold rock, unprotected apparently by its thin atmosphere, it is not possible to say. We know that the sun's action is strong enough to develop that immense train, the comet's tail, that sometimes spans our heavens. It is broken in pieces. We have seen the portions go off from the sun, to come back, probably, as separate comets. Solid fragments are scattered from it to travel in their own independent orbits. What is the condition of the burnt and crackled surface of a cometic mass or fragment as it goes out from the sun again into the cold? What changes may not that surface undergo before it comes back again, to pass anew through the fiery ordeal? We have here forces that we know are acting. They are intense, and act under varied conditions. The stones subject to those forces can have a history

full of all the scenes and actions required for the growth of such strange bodies as have come down to us. Some of our meteors, those of the star-showers, have certainly had that history. What good reason is there for saying that all of them may not have had the like birthplace and life?

The pieces which come into our air in any recurring star-shower belong to a group whose shape is only partly known. It is thin, for we traverse it in a short time. It is not a uniform ring, for it is not annual, except possibly the August sprinkle. How the sun's unequal attraction for the parts of a group acts as a dispersive force to draw it out into a stream, those most beautiful and most fruitfull discussions of Signor Schiaparelli have shown. The groups that we meet are certainly in the shape of thin streams.

It has been assumed that the cometic fragments go continuously away from the parent mass, so as to form, in due time, a ring-like stream of varying density, but stretched along the entire elliptic orbit of the comet. The epochs of the Leonid star-showers in November, which have been coming at intervals of thirty-three years since the year 902, have led us to believe that this departure of the fragments from Tempel's comet (1866, I.) and the formation of the ring was a very slow process. The meteors which we met near 1866 were therefore thought to have left the comet many thousands of years ago. The extension of the group was presumed to go on in the future until, perhaps tens of thousands of years hence, the earth was to meet the stream every year. Whatever may be the case with Tempel's comet and its meteors, this slow development is not found to be true for the fragments of Biela's comet. It is quite certain that the meteors of the splendid displays of 1872 and 1885 left the immediate vicinity of that comet later than 1840, although at the time of those showers they had become separated two hundred millions of miles from the computed place of the comet. The process, then, has been an exceedingly rapid one, requiring, if continued at the same rate, only a small part of a millennium for the completion of an entire ring, if a ring is to be a future form of the group.

It may be thought reasonable in view of this fact about Biela's comet, established by star-showers of 1872 and 1885, to revise our conception of the process of disintegration of Tempel's comet also. The more brilliant of the star-showers from this comet have always occurred very near the end of the thirty-three year period. Instead of there being a slow process which is ultimately to produce a ring along the orbit of the comet, it certainly seems more reasonable to suppose that the compact lines of meteors which we met in 1866, 1867, and 1868 left the comet at a recent date. A thousand years ago this shower occurred in the middle of October. By the precession of the equinoxes and the action of the planets the shower has moved to the middle of November. One-half of this motion is due to the precession, the other half to the perturbing action of the planets. Did the planets act upon the comet before the meteoroids left it, or upon the meteoroid stream? Until one has reduced the forces to numerical values, he may not give to this question a positive answer. But I strongly suspect that computations of the forces will show that the perturbations of Jupiter and Saturn upon that group of meteoroids hundreds of millions of miles in length,—perturbations strong enough to change the node of the orbit fifteen degrees along the ecliptic,—would not leave the group such a compact train as we found it in 1866. If this result is at all possible, it is because the total action is scattered over so many centuries. But it seems more probable that the fragments are parting more rapidly from the comet than we have assumed, and that, long before the complete ring is formed, the groups become so scattered that we do not recognize them, or else are turned away so as not to cross the earth's orbit.

Comets, by their strange behavior and wondrous trains, have given to timid and superstitious men more apprehensions than have any other heavenly bodies. They have been the occasion of an immense amount of vague and wild and valueless speculation by men who knew a very little science. They have furnished a hundred as yet unanswered problems which have puzzled the wisest. A world without water, with a strange and variable envelope which takes the place of an atmosphere, a world that travels repeatedly out into the cold and back to the sun, and slowly goes to pieces in the repeated process, has conditions so strange to our experience, and so impossible to reproduce by experiment, that our physics cannot as yet explain it. But we may confidently look forward to the answer of many of these problems in the future. Of those strange bodies, the comets, we shall have far greater means of study than of any other bodies in the heavens. The comets alone give us specimens to handle and analyze. Comets may be studied, like the planets, by the use of the telescope, the polariscope, and the spectroscope. The utmost refinements of physical astronomy may be applied to both. But the cometary worlds will be also compelled, through these meteorite fragments, —with their included gases and peculiar minerals, —to give up some additional secrets of their own life, and of the physics of space, to the blowpipe, the microscope, the test-tube, and the crucible.